Rotation of a Lamina Falling in Air.

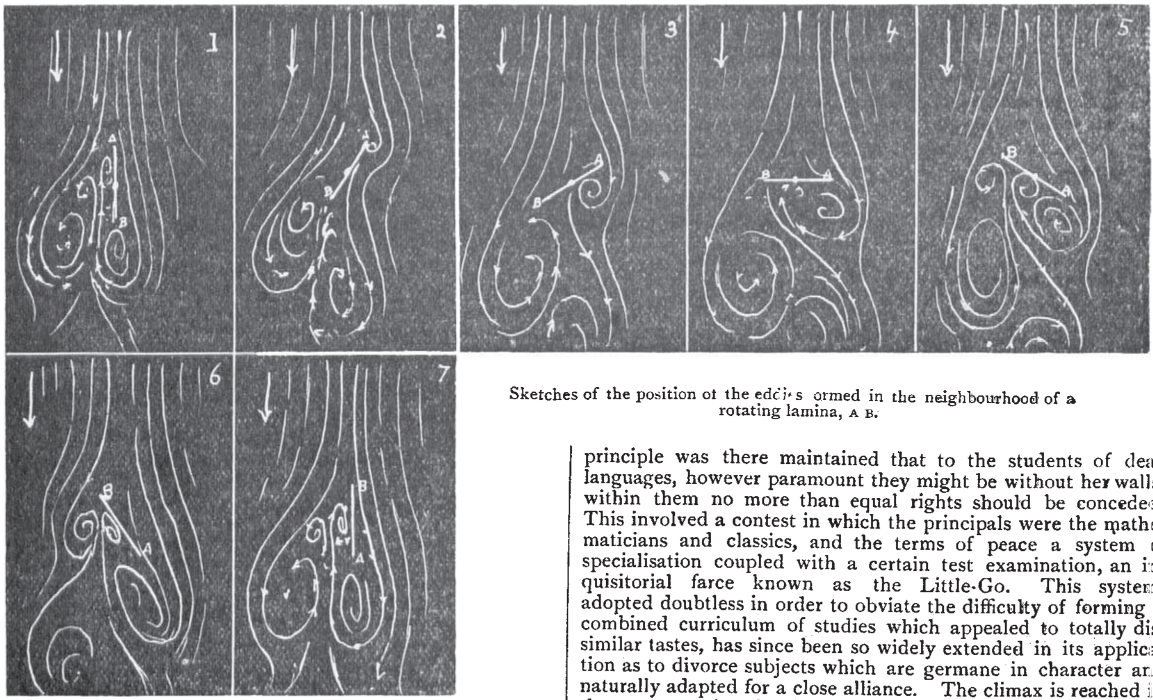
IN your issue of March 20, Dr. Johnstone Stoney, in reference to the behaviour of ice spiculæ in the clouds, instances the spinning of a card when dropped through the air.

I think Lord Rayleigh was the first person to point out how curious this phenomenon is and to show that the axis of a spinning lamina might be held between bearings without affecting the result. Also that a lamina so held and placed in a draught was equally ready to spin in either direction, thus precluding the idea that the rotation might be due to some want of symmetry in the lamina itself.

A few years ago I made some experiments on the rotation of laminæ in air currents.

The laminæ were mounted in bearings as frictionless as I could make them, and the experiment consisted (*a*) in measuring the speed of the air and the angular rotation of the lamina, (*b*) in mapping the flow of the air past the lamina. This was done by the use of smoke and intermittent illumination.

It would take too long to describe the apparatus in detail, but some of the results may be of sufficient interest for publication.



Sketches of the position of the eddies formed in the neighbourhood of a rotating lamina, A B.

Thus, the angular velocity of the lamina was found to be (through a considerable range of air speed, v, and breadth of lamina, b) $= \text{constant} \times \frac{v}{b}$.

There is a limit, however, to the smallness of the lamina which will revolve (*e.g.* small laminæ of gold-leaf a tenth of an inch long and two or three hundredths broad do not revolve, but oscillate as they fall).

The cause of the rotation depends on the way in which the eddies are formed on the downstream side of the lamina.

The changes which occur, in the eddy making, during half a revolution are shown in the accompanying sketches.

From these it will be seen that the spin causes the air to flow unsymmetrically with regard to the axis of the lamina, and that there will be, on the whole, a transverse force on the axis tending to press it in the direction of rotation of the leading edge.

It can be seen also that *before* the lamina reaches the positions 1 or 7, there will be a couple acting on it tending to accelerate the rotation, and that the couple will certainly be positive from position 1 to 3. It may be negative from between positions 3 and 4 to 6; but at any rate for more than half the total time the rotation is being accelerated.

To calculate the magnitude of the couple *a priori* is, I believe, beyond the power of mathematical analysis at present.

The experiments just alluded to were made with light rectangular laminæ, but these conditions are not essential—pennies spin very nicely when dropped from a great height.

I have not succeeded in making a lamina spin in a current of water, probably because the densities of the fluid and solid are not sufficiently different, but if a flywheel were fixed on the axis of the lamina, so as to be out of the water, and thus increase the moment of inertia of the turning body without altering the fluid friction, &c., rotation might perhaps be obtained in this case also.

A. MALLOCK.

March 22.

Mathematics and Science at Cambridge.

TO those who have at heart the advancement of scientific knowledge in Great Britain it is impossible but to acknowledge that the country owes a profound debt of gratitude to the University of Cambridge. During the dark ages of education her colleges formed the stronghold of modern culture; and the principle was there maintained that to the students of dead languages, however paramount they might be without her walls, within them no more than equal rights should be conceded. This involved a contest in which the principals were the mathematicians and classics, and the terms of peace a system of specialisation coupled with a certain test examination, an inquisitorial farce known as the Little-Go. This system, adopted doubtless in order to obviate the difficulty of forming a combined curriculum of studies which appealed to totally dissimilar tastes, has since been so widely extended in its application as to divorce subjects which are germane in character and naturally adapted for a close alliance. The climax is reached in the ever-growing barrier which separates mathematics from natural science, and which is year by year made more impassable by the examinations for entrance scholarships. These are of high value; and without this aid from the funds which the college authorities hold in trust for educational purposes many a brilliant man would be unable to consummate his work at school by a university career. It is, therefore, necessary that school curricula should be arranged in strict accordance with their requirements.

In view of their future careers, schoolboy mathematicians are divisible roughly into three classes—those who will make a profession of mathematics, those who will apply mathematics to science, and those who will, after obtaining academic honours, entirely sever their connection with the subject. To the last of these the question of syllabus is of comparatively small importance. But to the first and second it is of the utmost importance, and especially to the second, as it is essential to a man who for several years studies mathematics with an ulterior object in view that, while he should acquire a thorough grasp of the principles of the subject, he should not be forced by examining bodies to apply himself to what can only be regarded as intellectual pastimes; nor even in the case of the first can great skill in work of this class rank *pari passu* with a grasp of principle.

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Consider the case of an able schoolboy who makes mathematics his chief study between the ages, say, of fifteen and nineteen; this period should suffice to give him a working knowledge of three-dimensional analysis, integral calculus and differential equations with their applications to the mechanics of solids, in addition to the subjects now required by the colleges, were it not for certain side issues which have arisen from the latter and assumed stupendous proportions. Foremost among these stands the excessive measure of attention given to the analysis of curves of the second degree. These form naturally a small section in a treatise on analytical geometry; yet while the far-reaching principles of the latter are lightly passed over, there is scarcely a method so abstruse or an artifice so petty that it may not give rise to a question in an entrance scholarship paper if its bearing be upon conic sections; so that no less than a third of a schoolboy's mathematical hours are frequently sacrificed to these curves. Their real interest is due, firstly, to their appearance in astronomy (the properties there required are few, and might be treated in the appendix of a text-book on dynamics), and, secondly, to their adaptability to treatment by pure geometry, the direct methods of which lend them an interest which is alien to every other branch of mathematics, and supply a mental discipline which is elsewhere unattainable. On this alone rests their title to rank as a separate subject.

But this, though the most aggravated, is not the only instance of the kind. The time spent on advanced trigonometry is out of all proportion to its practical or educational value; the subject contains an introduction to the use of the complex quantity which properly forms part of the theory of functions, and includes a number of highly specialised problems which have no place in a syllabus so elementary as to exclude the integral calculus. A similar waste is caused by several large classes of questions, especially some on series, the ability to solve which implies nothing but an effort of memory, and by other fragments of high subjects which are thrust upon boys at an early stage and made artificially difficult by isolation from their proper context.

The above criticisms are not made in any spirit of hostility to examiners, who in framing their papers are trying to obviate the shortcomings of a system which is out of date and in need of radical reform. Five decades ago it was doubtless advisable, in view of the then state of school mathematics, to limit the entrance scholarship course to a few subjects, but under new and improved conditions is not an extension of syllabus preferable to the interweaving with these of a quantity of matter which, valuable as it may be to the problem setter, is for the student little else than a barrier to his progress?

But if the examinations in mathematics and science are to be brought into harmony, it is not only in the former that a new regime is needed. Under present conditions a boy could learn at school nearly the whole of the mathematics necessary for an advanced physical course. But to do this would jeopardise his chance of obtaining a first class in the science tripos unless the authorities recognised the value of his mathematical training by a substantial equivalent of marks, so that he might make a mathematical entrance scholarship the preliminary to a science course. For those who have not a taste for theory it would still be possible to adopt an experimental regime from the outset, though it is questionable whether they could in the long run bear comparison with men who had cultivated a faculty which it is no exaggeration to call a sixth sense. The glory of Cambridge and her highest traditions are centred in the names of her mathematical physicists. But the environment which surrounded Maxwell and Kelvin no longer exists. And to-day the growth of subjects and the consequent tendency to specialisation have gone far to materialise the phantom barrier which separates the practical from the theoretical, and have laid on the authorities the burden of creating a new system which shall be capable of reproducing the giants of the past.

Dulwich College, March 13. C. A. RUMSEY.

The Morphology of Pleuronectidæ.

IN the *Memoir* on the Plaice by Messrs. Cole and Johnstone, reviewed in NATURE of March 20 (p. 459), there is a reference to Steenstrup's work on the metamorphosis of Pleuronectidæ which I think the reviewer would have done well to correct. The statement to which I refer is the following:—"This supposition (that the left eye passed through the substance of the head to reach the ocular side), absurd as it may seem to us now, was in fact believed by such an observer as Steenstrup." The truth of the matter is that Steenstrup did not believe any supposition, absurd or otherwise, on the subject, but stated from actual observation that in certain larval Pleuronectidæ the eye of one side passed through the tissues of the head and emerged on the other side. The form in question was long known as Plagusia, and is now known to be the larva of Rhamboidichthys. The truth of Steenstrup's observations was fully confirmed by Alexander Agassiz at Newport, R.I.

On the other hand, in the "Story of Life in the Seas," by Prof. S. J. Hickson, it is stated that in the young of the soles the eye of one side passes through the head to the other side. This is equally incorrect.

Messrs. Cole and Johnstone also state that the Gadidæ are the nearest relatives of the Pleuronectidæ, and promise to show that the morphological differences between the cod and the plaice, apart from the question of symmetry, are comparatively few and unimportant. I cannot find in the *Memoir* that they have redeemed this promise, and the morphological similarity of the two forms is contradicted by the authors themselves on p. 185 of the *Memoir*. I believe it could be shown by proper investigation that the morphological differences between these two fish are numerous and profound.

I think some explanation was due from the reviewer concerning Dr. Woodward's "startling discovery" that in Cretaceous times Teleostei of the Clupeoid type had already translocated the pelvic fin into the jugular position. I have not yet had the pleasure of seeing Dr. Woodward's latest volume, but a Clupeoid with jugular fins seems to me indeed a startling discovery.

Penzance, March 22. J. T. CUNNINGHAM.

WHEN writing I was well aware that the sentence Mr. Cunningham quotes was not happily construed, but seeing that comment (like that of Mr. Cunningham himself) would needs be hypercritical, I allowed the matter to pass, in consideration of the context and of the desire to keep my review within bounds.

Concerning the Cretaceous teleostean described by Dr. Smith Woodward, I can only express my surprise that Mr. Cunningham should be so neglectful of the current literature of his subjects as to be unaware of its occurrence. With the rest of his letter I have no concern.

THE WRITER OF THE REVIEW.

Sun Pillars.

I HAVE been deeply interested in the correspondence in NATURE bearing on the appearance of sun pillars, and particularly so in the communication of Prof. Johnstone Stoney (p. 465).

The display of March 6 mentioned by the Rev. Guy Bridges and Mr. W. A. Knight was not visible here; but a very noteworthy occurrence of the phenomenon took place here on December 3 last and another on January 7 last, and there was one feature common to both of these events which has not been mentioned as occurring in relation to those recorded by other observers, so far as I am aware, but which seems to me to be a prime factor in the causation of the phenomenon under consideration, and it is this:—Before, and after, the actual time of the setting of the sun, during my own observations, there was a strong display of cirrus cloud in the "true cirrus" form of parallel bars, which appeared to run at right angles to the track of the setting sun, and roughly parallel, of course, to the horizon. As the setting sun neared the horizon, these bands of cloud became magnificently iridescent, displaying the spectrum colours of the rainbow, with the red nearest the horizon. As the sun set further the colours faded, beginning first at the red and following on in succession to the violet until this colour alone remained visible. Then ascended a beautiful pillar of a violet colour in exquisite shades, ascending gradually to a height of somewhere about twenty degrees above the horizon and then fading away slowly.

It seemed to me that the occurrence depended on (*a*) the presence of cirrus cloud (ice spiculæ); (*b*) these clouds must be in a banded or striated form; (*c*) they must lie at right angles to the track of the sun; and (*d*) these clouds must, of course, be present in the immediate neighbourhood of the setting sun. There are, besides, certain atmospheric conditions which seem to be desiderated in order to favour opacity, as dryness of the air, with calmness in its movements, and, I believe, a comparatively high barometric pressure. At the time of my

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